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led to Nobel prizes, clearly underscoring their intrinsic scientific merit. But both of these discoveries were made with modest equipment, at modest expense. The discovery of pulsars was carried out largely through the efforts of then graduate student Jocelyn Bell and four other students, spending two years banging wooden poles into the ground, stringing chicken wire between them, and recording the data on paper-chart recorders. No giant computers, no 100-million-dollar radio telescopes and multi-million dollar crash programs there. Just hard work, intelligence, imagination—and a bit of luck. But luck seems to be with those who are prepared for it, and who seize the day. If we look back over the history of radioastronomy, what we find is that many of the great steps were made by the Karl Janskys and Grote Rebers, funding their research on a shoestring; or the Jocelyn Bells who persist in the face of sage advice that what they have found is unimportant.

It is far too glib to suggest that money buys good ideas. This is not to say that "big science" is unnecessary, but that the intellectual edifice of science that Burke refers to is not built on millions of dollars, but on the insights, ideas and creativity of individuals. Let us hope that in the face of increasingly concentrated money in a few giant research institutions, the individuals with scientific ideas can still be heard above the din of the research factories, and be supported simply because of the ingenuity and beauty of their ideas.

KENNETH BRECHER
*Boston University
Boston, Massachusetts*

7/27/79

THE AUTHOR COMMENTS: Kenneth Brecher prefers, in his comments on my letter, to avoid practicality as an argument for science because it "debases both the practitioners and those who support it." This Brahmin view has not been universally agreed to by scientists. Galileo, Franklin, Gauss, Helmholtz, Kelvin, von Neumann, and Fermi are a few of the many practitioners who have expressed the opposite opinion. Science and technology are closely linked, and the aesthetics of science are not tainted when practical applications are found. No rash promises for practical benefits need be made, because history shows that the applications come in unexpected forms from unexpected sources, but the mutually beneficial exchange has been continuous for the last two centuries, and shows little sign of slackening.

In his concern for the heavy demands of big science, and radioastronomy in particular, Brecher is more seriously mistaken. When pioneering work is done with modest means, as in the discovery of pulsars, we can all take pleasure in the

elegance of the work. Yet, from the days of Tycho Brahe, it has from time to time been obvious that expensive equipment was needed to get the data so that theorists have facts to preserve them from error. At the opening of our own century, George Ellery Hale paved the way for modern astronomy by building a series of telescopes of extravagant size, and just recently Jan Oort persuaded the Dutch government to spend a very large sum on the Westerbork Synthesis Radio Telescope, with very little local support, and that instrument has already yielded a wealth of new insights into the nature of the cosmos. When the flux of photons is small, the collecting aperture must be large, and size is expensive. Brecher is also mistaken in his notions of size: The discovery of the 3 K cosmic background was not small science. Penzias and Wilson inherited the finest cryogenic maser receiver and the best calibrated antenna in the world, backed by the massive resources of Bell Labs. The discovery of quasars required still greater resources: Initial positions were measured with the Owens Valley interferometer of Cal Tech (the largest ONR program in US radio astronomy), complemented by Hazard's radio occultation work at the great 210-ft dish of CSIRO in Australia (the world's largest). The full story was clinched by optical observations with the 200-inch telescope at Mt. Palomar. No backyard science there!

BERNARD F. BURKE

Massachusetts Institute of Technology

7/25/79

Soviet vs. US referees

You published my letter "PRL versus JETP" and the "PRL Comments," which I find very demonstrative (December, page 82). At the end of the comments, the PRL editors inquire:

"... our authors practically never accept the criticism of the referee. Why that difference? [between PRL and JETP Letters—M.A.]. Are Russian referees more precise and more acquiescent? Are the editors of JETP firmer in their rejections than we are (or can be)?" The answers become obvious, if one questions: What happens, if the referee detains his comments for more than two weeks? Or if the referees essentially contradict each other, so that at least one of them is definitely wrong? Or if the paper is certainly novel, but the referee doubts its influence on further research? Or the author disagrees with the referees' specific criticism? Or the referees' comments are imprecise, or wrong due to his irresponsibility?

The situation in JETP and JETP Letters is as follows: If the comments refer to the style and are specific, any author does his best readily and quickly, because this leads to immediate publica-

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tion after the referee's OK. Certainly, the Russian and Western ideas of excellence in style are very different, but this is another problem.

Novelty goes as the top priority: really novel papers are too rare to overflow the journal and maybe too important to be rejected due to a subjective opinion of the referee on their future influence.

The priority of the urgency is very high: It is considered senseless to publish a short communication in *JETP Letters* after the time-lag, which allows for a full-length publication in *JETP*, and it is considered improper to cause such a delay for a new discovery or a high-current-interest paper in a rapidly changing field. That is why, in particular, the delay in the submitting of the referee's comments or their impreciseness is discussed and considered a disgrace by the editorial board (whose meetings are regular and include the best Soviet scientists in all fields of physics), refereeing is a part of the social duties of a physicist.

The same authoritative board meeting solves any conflict between the referee and the author or another referee. No referee and no author wants to expose his ignorance, incompetence or irresponsibility to this Board!

The result of such a system is obvious: the scientific truth is achieved in the first "round"; there remains no place for long correspondence with the author; the costs of the Journal are minimal (30 Kopecks per *JETP Letters* copy; that is about \$1.70 per month with no page charges).

To summarize: Yes, I do think Russian referees are more precise and more responsible! Yes, I do think this is a challenge to Western physicists and editors!

MARK AZBEL

Tel-Aviv University
Tel-Aviv, Israel

5/8/79

In a recent letter Mark Azbel showed that the Soviet editorial procedures yield demonstrably better results than American procedures (December, page 82). It seems to me that in their reply the editors of *Physical Review Letters* missed an opportunity to make a plea for improvements in the American editorial review system. Instead, they seem to say, among other things with which I agree, that the Soviet postal service is better (!), or that the Soviet physicists differ from American physicists in some way. Both views are certainly not so, but the latter contradicts present wisdom, which holds that all hominids are, statistically, isomorphous. We learn about *ourselves* when we study Soviet physicists, just as we learn about ourselves when we study Margaret Mead's Samoans.

The American system worked well when it served, say, 3000 physicists, and

personal relationships were able to give editors a measure of control. (I have a theorem that says that humans can identify with 3000 people, or less. The pragmatic proof is found in the fact that the Bible mentions only 3000 persons.) With 30 000 physicists there is simply no useful pressure that the editors can exert to help them obtain prompt and responsive reviews.

The Soviet system, which has also proven successful when used by other journals (*The International Journal of Electronics* is an example), evidently gives the editors sufficient clout to demand and get prompt and responsive reviews by enabling the editors to dispense honoraria for useful reviews. With the present American system one cannot be sure that in all cases there is a sufficient incentive to make a reviewer want to spend the time necessary for rapid preparation of the substantive and responsive review that Azbel feels the Soviet system yields. I find that to be told, by a referee who takes six weeks to get around to doing so, that I have the wrong attitude makes the American review process diverge. I grant that many reviewers will reply rapidly and well, but I see no reason to believe all reviewers can be expected to do so. I do not feel that altruism is universally distributed in sufficient quantity to be controlling in all cases. And professional self-interest can be a consideration in a group of 3000, but it is going to vary too greatly as a motivation to be a reliable consideration in a group of 30 000. Political self-interest should drive people to vote, but 100% do not vote. Any universal incentive, honoraria or whatever, will surely yield better reviews.

The essence of the problem is removing the bad cases. The bulk of the papers are reviewed rapidly and well, but the tragedy lies in those that are not. A legal system that unjustly executes 5% of its defendants can be a source of satisfaction to 95% of the population, but it is nevertheless a disaster to 5% of the population. Unfortunately, it is very difficult to make the plight of the few seem sufficiently important for the majority to take action.

Human institutions are not generally measured by their performance in the mean, but by their performance in the individual case. Each individual must be assured justice. The oldest remaining written legal code, that of the Babylonian Hammurabi, 2000 BC, is not concerned with the average honesty of witnesses but with the means of ensuring that no witness will be dishonest. Authors have one paper subject to editorial review, not a statistical ensemble. The imperative must therefore be to obtain for *each* paper prompt and responsive reviews that have an intellectual content commensurate with the author's measure of the intellectual content of his paper.

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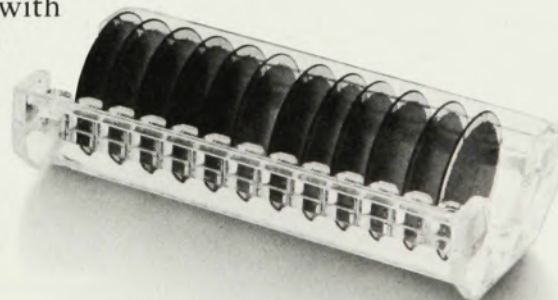
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A discussion of the whole editorial control process is too vast in reach to attempt here. The point is that it is time that AIP and/or APS begin a real effort to review and improve the present editorial review system.

Human institutions seem to operate with dynamic stability. This wisdom is the message in the well-known paradoxical amorphisms: "*Plus ça change, plus c'est la même chose*", or the Sicilian, "If we want things to remain the same, things will have to change." My own version, which applies particularly to the academic world, is, "The problems remain the same, only the solutions change"! So I am not asking for change because the present system is without merit, but, on the contrary, because it *has* merit. And it is only with change to accommodate to a changing world that one can hope to keep it the same!

I realize that this is a concern of the minority and that most physicists have developed some mechanism to cope with the present system, but if those of the small minority who are tired of trying to make it work will write to me we can see what can be done!

M. W. P. STRANDBERG

Massachusetts Institute of Technology
2/27/79 Cambridge, Massachusetts

THE EDITOR OF PRL COMMENTS: In our response to Mark Azbel's first letter (December, page 83), we suggested that important differences between *Physical Review Letters* and *JETP Letters* followed from "appreciable differences between the expectations of authors, referees and readers of these two, nominally similar, journals." To us, Azbel's second letter suggests how these differences in the journals may stem from considerable differences between Soviet society and Western society. I doubt that we could construct (even aside from logistic difficulties) an "authoritative" board whose opinions would be always accepted by referees and authors. We note that our costs to a member of about 50 cents an issue are not very different from the *JETP Letters* price of 30 Kopecks an issue. The difference in page charges must represent the difference between a subsidized and an unsubsidized journal. As to whether Russian referees are more precise and responsible than *Physical Review Letters* referees, we do not have enough information to comment responsibly.

Strandberg states that, "Azbel showed that the Soviet editorial procedures yield demonstrably better results than American procedures." We understood that Azbel demonstrated that Soviet procedures yielded *quicker* results. There are other dimensions to journals than speed of publication. Strandberg's statement concerning the "present wisdom," which,

we gather, considers that Western society, Soviet society and Samoan society are "isomorphous," we pass over. His, and implicitly, Azbel's, suggestion concerning honoraria is interesting but we doubt that it would be financially feasible for *Physical Review Letters*. We use about 8000 referees' reports a year. Our page-charge receipts of about \$300 000 a year and our receipts from member subscriptions of about \$225 000 set the scale of our economics. We doubt that an honorarium of less than \$50 would have any effect on responses, and that cost would be prohibitive.

Our strong disagreement with some of the specific statements in the letters of Azbel and Strandberg tend to obscure our broad agreement with their implication that *Physical Review Letters*, though it is in many ways an excellent journal, has serious flaws and that the editors, the American Physical Society and the community in general should be concerned with these flaws. Certainly, over the past decade, the society and editors have responded to deep concerns the community has expressed over the journals of the society—in particular, *Physical Review Letters*—and the editors have made many changes in editorial procedures in response to that concern. We are always making modest changes and we occasionally propose major changes. Such a proposal for a radical change in the journal is now under preparation and will soon be presented to the community for its consideration.

ROBERT K. ADAIR
Editor

6/25/79

Physical Review Letters

US vs. USSR

I was attracted by the two complementary items about Linus Pauling in December—"Linus Pauling wins Lomonosov Gold Medal" (page 68) and the letter under the title "Pauling and Sakharov" (page 81). It appears to me that these two outstanding items can be misinterpreted to the detriment of our colleagues in the Soviet Union, who are being terrorized, humiliated and dehumanized. Surely Pauling's comparison of what happened to him during the McCarthy twilight era with the darkness in which many of our colleagues in the USSR are living today is an exaggeration. To compare the two governments as immoral on the same basis is surely an injustice to the democratic institutions and the freedoms we enjoy in North America. None of us in the United States and Canada today is dying a new death daily in concentration camps, jails and insane asylums like our colleagues Yuri Orlov, Alexander Bolonkin, Sergei Kovalev, Anatoly Shcharansky, Semyon Gluzman and the hundreds of others who are slowly being forgotten in the West.

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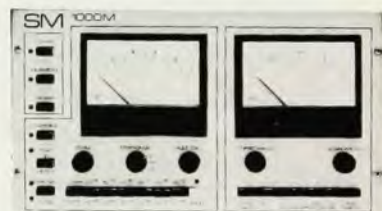
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